# PHILOSOPHICAL

# TRANSACTIONS.

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I. *The Bakerian Lecture. On some of the Combinations of Oxymuriatic Gas and Oxygene, and on the chemical Relations of these Principles, to inflammable Bodies. By* Humphry Davy, *Esq. LL. D. Sec. R. S. F. R. S. E. M. R. I. A. and M. R. I.*

Read November 15, 1810.

1. *Introduction.*

In the last communication which I had the honour of pre senting to the Royal Society, I stated a number of facts, which inclined me to believe, that the body improperly called in the modern nomenclature of chemistry, *oxymuriatic acid gas,* has not as yet been decompounded; but that it is a peculiar substance, elementary as far as our knowledge extends, and analogous in many of its properties to oxygene gas.

My objects in the present Lecture, are to detail a number of experiments which I have made for the purpose of illustrating more fully the nature, properties, and combinations of this substance, and its attractions for inflammable bodies, as compared with those of oxygene; and likewise to present

some general views and conclusions concerning the chemical powers of different species of matter, and the proportions in which they enter into union.

I have been almost constantly employed, since the last session of the Society, upon these researches, yet this time has not been sufficient to enable me to approach to any thing complete in the investigation. But on subjects, important both in their connexion with the higher departments of chemical philosophy, and with the œconomical applications of chemistry, I trust that even these imperfect labours will not be wholly unacceptable.

### 2. *On the Combinations of Oxymuriatic Gas and Oxygene, with the Metals from the fixed Alkalies.*

The intensity of the attraction of potassium for oxymuriatic gas, is shewn by its spontaneous inflammation in that substance, and by the vividness of the combustion. I satisfied myself, by various minute experiments, that no water is separated in this operation, and that the proportions of the compound are such that one grain of potassium absorbs about 1.1 cubical inch of oxymuriatic gas at the mean temperature and pressure, and that they form a neutral compound, which undergoes no change by fusion. I used, in the experiments from which these conclusions are drawn, a tray of platina for receiving the potassium; the metal was heated in an exhausted vessel, to decompose any water absorbed by the crust of potash, which forms upon the potassium during its exposure to the atmosphere, and the gas was freed from vapour by muriate of lime. Large masses of potassium cannnot be made

to inflame, without heat in oxymuriatic gas. In all experiments in which I fused the potassium upon glass, the retorts broke in pieces in consequence of the violence of the combustion, and even in two instances when I used the tray of platina. If oxymuriatic gas be used, not freed from vapour, or if the potassium has been previously exposed to the air, a little moisture always separates during the process of combustion. When pure potassium, and pure oxymuriatic gas are used, the result, as I have stated, is a mere binary compound, the same as muriate of potash, that has undergone ignition.

The combustion of potassium and sodium in oxygene gas, is much less vivid than in oxymuriatic gas. From this phenomenon, and from some others, I was inclined to believe that the attraction of these metals for oxygene is feebler, than their attraction for oxymuriatic gas. I made several experiments, which proved that this is the fact; but before I enter upon a detail of them, it will be necessary to discuss more fully than I have yet attempted, the nature of the combinations of potassium and sodium with oxygene, and of potash and soda with water.

I have stated in the last Bakerian Lecture, that potassium and sodium, when burnt in oxygene gas, produce potash and soda in a state of extreme dryness, and very difficult of fusion. In the experiments from which these conclusions are drawn, as I mentioned, I used trays of platina, and finding that this metal was oxidated in the operation, I heated the retort strongly, to expel any oxygene the platina might have absorbed, and except in cases when this precaution was taken, I found the absorption of oxygene much greater than could be accounted for by the production of the alkalies. In all

cases in which I burnt potassium or sodium in common air, applying only a gentle heat, I found that the first products were substances extremely fusible, and of a reddish brown colour, which copiously effervesced in water, and which became dry alkali, by being strongly heated upon platina in the air, phenomena, which, at an early period of the enquiry, induced me to suppose that they were prot-oxides of potassium and sodium. Finding, in subsequent experiments, however, that they deflagrated with iron filings, and rapidly oxidated platina and silver, I suspended my opinion on the subject, intending to investigate their nature more fully.

Since that time, these oxides, as I find by a notice in the Moniteur for July 5th, 1810, have occupied the attention of M. M. GAY LUSSAC and THENARD, and these able chemists have discovered that they are peroxides of potassium and sodium, the one containing, according to them, three times as much oxygene as potash, and the other 1.5 times as much as soda

I have been able to confirm in a general way these interesting results, though I have not found any means of ascertaining accurately, the quantity of oxygene contained in these new oxides. When they are formed upon metallic substances, there is always a considerable oxidation of the metal, even though platina be employed. I have used a platina tray lined with muriate of potash, that had been fused; but in this case, though I am inclined to believe that some alkali was formed at the same time with the peroxides, yet I obtained an absorpton of 2.6 cubical inches, in a case when 2 grains of potassium were employed, and of 1.63 cubical inches, in a case when a grain of sodium was used, but in this last instance, the edge of the platina tray had been acted upon by the metal,

and was oxidated.* The mercury in the barometer in these experiments stood at 30.12 inches, and that in the thermometer at 62° FAHRENHEIT.

When these peroxides were formed upon muriate of potash, the colour of that from potassium was of a bright orange; that from sodium of a darker orange tint. They gave off oxygene, as M. M. GAY LUSSAC and THENARD state, by the action of water or acids. They were converted into alkali, as the French chemists have stated, by being heated with any metallic or inflammable matter. They thickened fixed oils, forming a compound that did not redden paper tinged with turmeric, without the addition of water.

When potassium is brought in contact with fused nitre, in tubes of pure glass, there is a slight scintillation only, and the nitre becomes of a red brown colour. In this operation, nitrogene is produced, and the oxide of potassium formed. I thought that by ascertaining the quantity of nitrogene evolved by the action of a given weight of potassium, and comparing this with the quantity of oxygene disengaged from the oxide by water, I might be able to determine its composition accurately. A grain of potassium acting in this way, I found produced only $\frac{16}{100}$ of nitrogene; and the red oxide by its action upon water, produced less than half a cubical inch of oxygene, so that it is probable that potash as well as its peroxide is formed in the operation.

* M. M. GAY LUSSAC and THENARD have stated in the paper above referred to, that common potash and barytes absorb oxygene when heated. It would seem that the action of the fixed alkalies, and of barytes on platina, depends on the production of the peroxides. I have little doubt but that these ingenious gentlemen will have anticipated this observation, in the detailed account of their experiments.

Sodium, when brought in contact with fused nitre, produced a violent deflagration. In two experiments in which I used a grain of the metal, the tube broke with the violence of the explosion. I succeeded in obtaining the solid results of the deflagration of $\frac{1}{2}$ a grain of sodium, but it appeared that no peroxide had formed, for the mass gave no oxygene by the action of water.

When potassium is burnt in a retort of pure glass, the result is partly potash and partly peroxide, and by a long continued red heat the peroxide is entirely decomposed.

A grain of potassium was gently heated in a small green glass retort containing oxygene; it burnt slowly, and with a feeble flame; a quantity of oxygene was absorbed equal to $\frac{90}{100}$ of a cubical inch; by heating the retort to dull redness, oxygene was expelled equal to $\frac{31}{100}$ of a cubical inch; the mercury in the thermometer in this experiment stood at 63° FAHRENHEIT, and that in the barometer at 30.1 inches.

In experiments on the electrical decomposition of potash and soda, when the VOLTAIC battery employed contains from 500 to 1000 series in full action; the metals burn at the moment of their production, and form the peroxides; and it is probable, from the observations of M. RITTER, that these bodies may be produced likewise in VOLTAIC operations on potash, at the positive surface.

In my early experiments on potassium and sodium, I regarded the fusible substances appearing at the negative surface, in the VOLTAIC circuit, as well as those produced by the exposure of the metals to heat and air, as prot-oxides, and as similar to the results obtained by heating the metals in contact with small quantities of alkali.

I have repeated these last operations, in which I conceived that prot-oxides were formed.

Potassium and sodium, when heated in glass tubes in contact with about half of their weight of potash and soda, that have been ignited, become first of a bright azure, then produce a considerable quantity of hydrogene, and at last form a gray coherent mass, not fusible at a dull red heat, and which gives hydrogene by the action of water.

Whether these are true prot-oxides, or merely mixtures of the alkaline metals with the alkalies, or with the alkalies and reduced silex from the glass, I shall not at present attempt to decide.

Potassium I find heated in a similar manner with fused potash, in a tube of platina, gives after having been ignited, a dark mass that effervesces with water; but even in this case, it may be said that the alloy of platina and potassium interferes, and that the substance is not a protoxide, but merely dry alkali mixed with this alloy.

As the pure alkalies were unknown, till the discovery of potassium and sodium,* and as their properties have never been described, it will perhaps be proper in this place to notice them briefly.

* STAHL approached nearly to the discovery of the pure alkalies. He cemented solid caustic potash with iron filings in a long continued heat, and states, that in this way an alkali "valde causticum" is produced. *Specim. Becb.* part ii. page 255. He procured caustic alkali also, by decomposing nitre by the metals. Id. p. 253.

I find that when nitre is decomposed in a crucible of platina, by a strong red heat, a yellow substance remains, which consists of potash and oxide of platina, apparently in chemical combination. The undecompounded potash which comes over in the process for procuring potassium by the gun-barrel, is of an olive colour, and affords oxide of iron during its solution in water. Pure potash will probably be found to have an affinity for many metallic oxides.

When potassium and sodium are burnt in oxygene gas upon platina, and heated to redness to decompose the peroxide of potassium, the alkalies are of a grayish green colour. They are harder than common potash or soda, and, as well as I could determine by an imperfect trial, of greater specific gravity. They require a strong red heat for their perfect fluidity, and evaporate slowly, by a still further increase of temperature. When small quantities of water are added to them, they heat violently, become white, and are converted into hydrats, and then are easily fusible and volatile.

When potassium or sodium is burnt on glass, freed from metallic oxides, and strongly heated, or when potash or soda is formed from the metals by the action of a minute quantity of water, their colour approaches to white; but in other sensible properties, they resemble the alkalies formed upon metallic substances; and are distinguished in a marked manner by their difficult fusibility from the potash and soda prepared by alcohol.

M. D'ARCET, and more distinctly M. BERTHOLLET, have concluded that the loss of weight of common fused potash and soda, during their combination with acids, depends upon the expulsion of water, which M. BERTHOLLET has rated at 13.9 per cent. for potash, and M. D'ARCET, at 27 or 28 for potash, and 28 or 29 for soda.*

I have stated in the last Bakerian Lecture, that my own results led me to conclude, that fused potash contained about 16 or 17 parts in the 100 of water, taking the potash formed, by adding oxygene to potassium as a standard.

The experiment from which I drew my conclusions, was made on the action of silex and potash fused together, and

* Annales de Chimie, tom. 68, page 190.

I regarded the loss of weight, as the indication of the quantity of moisture.

I am acquainted with no experiment on record, in which water has been actually collected from the ignited fixed alkalies, and this appeared necessary for the complete elucidation of the subject.

I heated together in a green glass retort, 40 grains of potash, (that had been ignited for several minutes), and 100 grains of boracic acid, which had been heated to whiteness for nearly an hour. The retort was carefully weighed, and connected with a small receiver, which was likewise weighed; the bulb of the retort was then gradually heated till it became of a cherry red; there was a violent effervescence in the retort, a fluid condensed in the neck, and passed into the receiver. When the process was completed, the whole of the retort was strongly heated; it was found to have lost $6\frac{1}{2}$ grains, and the receiver had gained 5.8 grains. The fluid that it contained was water, holding in solution a minute quantity of boracic acid, and when evaporated, it did not leave an appreciable quantity of residuum.

A similar experiment made upon soda, heated to redness, but in which the water collected was not weighed, indicated 22.9 of water in 100 parts of soda.

It may be asked, whether part of the water evolved in these processes might not have been produced from the boracic acid, or formed in consequence of its agency; but the following experiments shew that this can not be the case in any sensible degree.

I heated 8 grains of potassium, with about 50 grains of boracic acid, to redness in a tube of platina, connected with a

glass tube, kept very cool; but I found that no moisture whatever was separated in the process. I mixed a few grains of potassium with red oxide of mercury, and ignited the mixture in contact with boracic acid, but no elastic product, except mercury, was evolved.

I made some potash by the combustion of potassium in a glass tube, and ignition of the peroxide, I added to it dry boracic acid, and heated the mixture to redness. Sub-borate of potash was formed, and there was not the slightest indications of the presence of moisture.*

It is evident from this chain of facts, that common potash

* These processes must not however be considered as shewing that boracic acid that has been heated to whiteness is entirely free from water; they merely prove that such an acid gives off no water by combination with pure potash at a *red heat*. I have found that boracic acid in perfect fusion, and that has been long exposed to the blast of a forge, and that has long ceased to effervesce, gives globules of hydrogene; when dry iron filings are made to act upon it. I added to 54 grains of boracic acid in complete fusion, in a crucible of platina, 75 grains of flint glass that had been previously heated to whiteness, and immediately reduced into powder in a hot iron mortar; by raising the heat so as to produce combination, a copious effervescence was produced; and after intense ignition for half an hour, the mixture was found to have lost three grains and a quarter.

The combinations of boracic acid with potash and soda, that have been heated to redness, I find lose weight when their temperature is raised to a much higher degree. Thus, in an experiment made in the laboratory of my friend JOHN GEORGE CHILDREN, ESQ. and in which Mr. CHILDREN was so kind as to co-operate, 71 grains of hydrat of potash, mixed with 96 of boracic acid that had been heated as strongly as possible in a blast furnace, lost by fusion together in a red heat 11 grains, but on raising the temperature to whiteness the loss increased to above 13 grains. 55,5 grains of hydrat of soda, mixed with 80 of boracic acid, examined at intervals in a process of this kind, continued to lose weight for half an hour, during which time they were frequently heated to whiteness; at the end of this period the whole loss was 14 grains, of which at least one grain and a half may be referred to the acid. 95 grains of soda, ignited to whiteness in a platina crucible, with 140 of dry flint glass, lost 22.2 grains; 80 grains of boracic glass were added to this mixture; a fresh effer-

and soda are hydrats, and the bodies formed by the combustion of the alkaline metals, are, as I have always stated, pure metallic oxides, (as far as our knowledge extends) free from water.*

vescence took place, and after intense ignition for a few minutes, there was an additional loss of weight of four grains and a half. The energy with which water adheres to certain bodies in other cases, is shewn by the experiments of M. BERTHOLLET, *Mem. d'Arcueil*, tom. ii. page 47. Indeed it is impossible to say that a neutral compound, or a fixed acid is ever entirely free from water; it is only the first proportions that are easily separated. If the proportions of water in common potash and soda were to be judged of from their loss of weight, in combining with boracic acid, it would appear to be from 19 to 20 per cent. in the first, and from 23 to 25 in the second.

* After the experiments detailed in my two last papers, it may perhaps appear unnecessary, at least to those enlightened British chemical philosophers who have closely followed the progress of science, to offer any new evidences to prove that potassium and sodium are not hydrurets of potash and soda, particularly as M. M. GAY LUSSAC and THENARD, the ingenious advocates of this notion have acknowledged, in the Moniteur to which I have before referred, that it is not tenable; but on a subject so intimately connected with the most refined departments of chemical philosophy, and with so many new objects of research, additional facts cannot be wholly devoid of use and application.

Mr. DALTON, in the second volume of the work which he entitles "*A New System of Chemical Philosophy*," of which he has had the goodness to send me a copy, has, I find in his first pages, adopted the idea that potash and soda are metallic oxides; but in the latter pages has considered them as simple bodies, and the metals formed from them as compounds of potash and soda with hydrogene. He has given no facts in favour of this change in his opinion: his principal argument is founded upon the process in which I first obtained potassium. Common potash is a hydrat: when oxygene is procured from this by VOLTAIC electricity at one surface, and potassium at the other surface; Mr. DALTON conceiving that this oxygene arises from the water, states that the hydrogene of the water must combine with the potash to form potassium. It is evident, that adopting such a plan of reasoning, lead or copper might be proved to be hydrurets of their oxides; for when these metals are revived from their aqueous acid solutions, oxygene is produced at the positive surface, and no hydrogene at the negative surface.

In my first experiments for producing potassium and sodium, I used a weak

I shall now resume the detail of the experiments that I have made, on the relative attractions of oxymuriatic gas and oxygene, for the metals of the fixed alkalies. I burnt a grain of potassium in oxygene gas, in a retort of green glass, furnished with a stop-cock, and heated the oxide formed, to redness, to convert it into potash: half a cubical inch of oxygene was absorbed. The retort was exhausted, and very pure oxymuriatic gas admitted. The

power, and in these instances procuring the metals in very small quantities only, I perceived no effervescence. When from five hundred to one thousand plates are used for producing potassium, there is a violent effervescence, and a production of hydrogene and sometimes of potassuretted hydrogene, connected with the formation of the metal.

Potassium, brought in contact with red hot hydrat of potash, disengages abundance of hydrogene, and the whole is converted into difficultly fusible potash.

327 grains of hydrat of potash that had been ignited, were made to act in a gun-barrel on 745 grains of iron turnings heated to whiteness. Some hydrogene was lost, and some hydrat of potash remained undecompounded, yet 225 cubical inches of inflammable gas were collected, and 50 grains of potassium, and a large quantity of an alloy of potassium and iron formed, so that it is scarcely possible to doubt that all the hydrogene produced from the decomposed hydrat of potash was liberated.

Mr. DALTON conceives that there is an analogy between potassium and sodium, and the compounds of hydrogene with sulphur, phosphorus, and arsenic; but I am at loss to trace any similarity between sulphureted hydrogene, which is a gaseous body, soluble in water, and having acid properties, and a highly inflammable solid metal which produces alkali by combustion. Potassium might as well be compared to carbonic acid. Mr. DALTON considers the volatility of potassium and sodium as favouring the idea of their containing hydrogene; but they are less volatile than antimony, arsenic, and tellurium, and much less volatile than mercury. He mentions their low specific gravity as a circumstance favourable to this idea. I have on a former occasion examined this argument, first brought forward by M. RITTER; but it may not be amiss to add, that if potassium is a compound of hydrogene and potash, hydrat of potash must contain an equal quantity of hydrogene, with the addition of a light gaseous element, oxygene, which might be expected to diminish rather than to increase the specific gravity of the compound. Mr. DALTON states, p. 488, that potassium forms dry hydrat of potash, by decomposing nitrous gas

colour of the potash instantly became white, and by a gentle heat, the whole was converted into muriate of potash: a cubical inch and $\frac{1}{8}$ of oxymuriatic gas were absorbed, and exactly half a cubical inch of oxygene generated. The barometer during this operation was at 30.3, the thermometer at 62 FAHRENHEIT. I made several experiments of the same kind, but this is the only one on which I can place entire dependence. When I attempted to use larger quantities of potassium, the retort usually broke during the cooling of the glass, and it was not possible to gain any accurate results in employing metallic trays. The potassium was spread into a thin plate, and of course was much oxidated before its admission into the retort, which rendered the absorption of oxygene a little less than it ought to have been. In the process it was heated in vacuo before the combustion, to decompose the water in the crust of potash; for in cases when this precaution was not taken, I found that hydrat of potash sublimed, and lined the upper part of the retort, and from this the oxymuriatic gas separated water as well as oxygene.

The phenomenon of the separation of water from hydrat of potash by oxymuriatic gas, was happily exemplified in an experiment in which I introduced oxymuriatic gas to the peroxide of potassium, formed in a large retort, and in which

and nitrous oxide; this is not the case: and he does not refer to experiment. I find by some very careful trials, that potassium attracts the oxygene and some of the nitrogene from these bodies, and forms a fusible compound which may be decomposed, giving off nitrogene and its excess of oxygene, by a red heat, and which becomes *potash*, and not dry hydrat of potash.

M. M. GAY LUSSAC and THENARD have convinced themselves that potassium and sodium are not hydrurets of potash and soda, by a method similar to that which I adopted and published some months before, namely, by producing neutral salts from them.

the potassium had been covered with a considerable crust of hydrat of potash. The upper part of the retort and its neck contained a white sublimate of hydrat, which had risen in combustion, and which was perfectly opaque. As soon as the gas was admitted, it instantly became transparent from the evolution of water; and on heating the glass in contact with the sublimate, its opacity was restored, and water driven off.

In various cases in which I heated dry potash, or mixtures of potash and the peroxide, in oxymuriatic gas, there was no separation of moisture, except when the gas contained aqueous vapour; and the oxygene evolved in the process, when the heat was strongly raised, exactly corresponded to that absorbed by the potassium.

When muriatic acid gas was introduced to potash formed from the combustion of potassium, water was instantly formed, and oxymuriate of potassium.* I have made no accurate experiment on the proportions of muriatic acid gas decomposed by potash, but I made a very minute investigation, of the nature of the mutual decomposition of this substance, and hydrat of potash.

Ten grains of hydrat of potash were heated to redness in a tray of platina, which was carefully weighed; it was introduced into a retort which was exhausted of air, and the retort was filled with muriatic acid gas. The hydrat of potash was heated by a spirit lamp; water instantly separated in great abundance, and muriate of potash formed. A strong heat was applied till the process was completed, when the tray was taken out and weighed; it had gained $2\frac{13}{16}$ grains. A minute quantity of liquid muriatic acid was added to the muriate, to

* i. e. Muriate of potash.

ensure a complete neutralization, and the tray heated to redness: there was no additional increase of weight.

In the few experiments which I have made on the action of sodium and soda on oxymuriatic gas, the phenomena appeared precisely analogous; but sodium, as might have been expected, absorbed nearly twice as much oxymuriatic gas as potassium.

When common salt that has been ignited, is heated with potassium, there is an immediate decomposition, and by giving the mixture a red heat, pure sodium is obtained; and this process affords an easy mode, and the one I have always lately adopted for procuring that metal. No hydrogene is disengaged in this operation, and two parts of potassium I find produces rather more than one of sodium.

From the series of proportions that I have communicated in my last paper, it is evident that 1 grain of potassium ought to absorb 1.08 cubical inches of oxymuriatic acid; and that the potash formed from one grain of potassium ought to decompose about 2.16 cubical inches of muriatic acid gas; and these estimations agree very nearly with the result of experiments.

The estimation of the composition of soda, as deduced from the experiments in the last Bakerian lecture, is 25.4 of oxygene to 74.6 of metal, and this would give the number representing the proportion in which sodium combines with bodies 22.;* from which it is evident, that a grain of sodium

* Or if soda be considered as deutoxide, which seems probable from the experiments detailed page 4, 44; and on this supposion, the salts of soda must be conceived to contain double proportions of acid. On either datum the proportion of oxygene in water must be taken as 7.5, and that of hydrogene as 1, though other numbers might be found as divisors or multiples of those which would equally harmonise with the general doctrine of definite proportions. In my last communication to the Society, I have quoted Mr. Dalton as the original Author of the hypothesis,

ought to absorb nearly 2 cubical inches of oxymuriatic gas, and that the same quantity converted into soda, would decompose nearly four cubical inches of muriatic gas. Muriate of soda ought on this idea to contain one proportion of sodium, 22., and one of oxymuriatic gas 32.9; and this estimation is very near that which may be gained from Dr. MARCET'S analysis of this substance. Hydrat of potash ought to consist of 1 proportion of potash, represented by 48., and one of water, represented by 8.5. This gives its composition as 15.1 of water, and 84.9 of potash. Hydrat of soda ought, according to theory, to contain 1 proportion of soda 29.5, and 1 of water 8.5, which will give in 100 parts 22.4 of water; and the experiments that I have detailed, conform as well as can be expected with these conclusions.

The proportions of potash and soda indicated, in different neutral combinations, by these estimations, will be found to agree very nearly with those derived from the most accurate analysis, particularly those of M. BERTHOLLET; or the differences are such as admit of an easy explanation.

I stated in my last communication, the probability that the oxygene in the hyperoxymuriate of potash was in triple combination with the metal and oxymuriatic gas; the new facts

that water consists of 1 particle of oxygene, and 1 of hydrogene; but I have since found that this opinion is advanced, in a work published in 1789. *A comparative View of the Phlogistic and Antiphlogistic Theories,* by WILLIAM HIGGINS. In this elaborate and ingenious performance, Mr. HIGGINS has developed many happy sketches of the manner in which (on the corpuscular hypothesis) the particles or molecules of bodies may be conceived to combine; and some of his views, though formed at this early period of investigation, appear to me to be more defensible, assuming his data, than any which have been since advanced; for instance, he considered nitrous gas as composed of two particles of oxygene, and one of nitrogene. Mr. HIGGINS had likewise drawn the just conclusion respecting the constitution of

respecting the peroxide confirm this idea. Potassium, perfectly saturated with oxygene, would probably contain six

sulphuretted hydrogene, from its electrical decomposition. As hydrogene is the substance which combines with other bodies in the smallest quantity, it is perhaps the most fitted to be represented by unity; and on this idea the proportions in ammonia will be 3 of hydrogene to 1 of nitrogene, and the number representing the smallest proportion in which nitrogene is known to combine will be 13.4. Mr. DALTON, *New System of Chemical Philosophy*, pages 323 and 436, has adopted 4.7 or 5.1, as the number representing the weight of the *atom* of nitrogene; and has quoted my experiments, *Researches, Chemical and Philosophical*, as authorising these numbers; but all the enquiries on nitric acid, nitrous gas, nitrous oxide, and on the decomposition of nitrat of ammonia stated in that work, conform much more nearly to the number 13.4.

According to Mr. DALTON, nitrat of ammonia contains one proportion of acid and one of alkali, and nitrate of potash two proportions of acid and one of alkali; but it is easy to see that the reverse must be the case. Nitrate of ammonia is known to be an acid salt; and nitrate of potash a neutral salt; which harmonizes with the views above stated. Mr. DALTON estimates the quantity of water in nitric acid of specific gravity 1.54, at 27.5 per cent.; and this, according to him, is a stronger acid than he obtained by decomposing fused nitre by sulphuric acid, which contained only 19 per cent. of water, and one quantity of sulphuric acid, according to him, will produce from nitre, more than an equal weight of nitric acid, and he supposes no water in nitre; so that his conclusion as to the quantity of water in liquid nitric acid on his own data must be incorrect. I find water in fused nitre, by decomposing it by boracic acid.

I shall enter no further at present into an examination of the opinions, results, and conclusions of my learned friend; I am however obliged to dissent from most of them, and to protest against the interpretations that he has been pleased to make of my experiments; and I trust to his judgment and candour for a correction of his views.

It is impossible not to admire the ingenuity and talent with which Mr. DALTON has arranged, combined, weighed, measured, and figured his atoms; but it is not, I conceive, on any speculations upon the ultimate particles of matter, that the true theory of definite proportions must ultimately rest. It has a surer basis in the mutual decomposition of the neutral salts, observed by RICHTER and GUYTON DE MORVEAU, in the mutual decompositions of the compounds of hydrogene and nitrogene, of nitrogene and oxygene, of water and the oxymuriatic compounds; in the multiples of oxygene in the nitrous compounds; and those of acids in salts, observed by Drs. WOLLASTON

proportions; for, according to Mr. CHENEVIX's analysis, which is confirmed by one made in the Laboratory of the Royal Institution, by Mr. E. DAVY, hyperoxymuriate of potash must consist of 40.5 potassium, 32.9 oxymuriatic gas, and 45 of oxygene.

I have mentioned, that by strongly heating the peroxide of potassium in oxymuriatic acid, all the oxygene is expelled, and a mere combination of oxymuriatic gas and potassium formed. I thought it possible, that at a low temperature, a combination might be effected, and I have reason to believe that this is the case. I made a peroxide of potassium, by heating potassium with about twice the quantity of nitre, and admitted oxymuriatic gas which was absorbed: some oxygene was expelled on the fusion of the peroxide, but a salt remained, which gave oxymuriatic gas, as well as muriatic acid, by the action of sulphuric acid.

It seems evident, that in the formation of the hyperoxymuriate of potash, one quantity of potash is decomposed by the attraction of oxymuriatic gas to form muriate of potash; but the oxygene, instead of being set free in the nascent state, enters into combination with another portion of potash, to form a peroxide, and with oxymuriatic gas.

The proportions required for these changes may be easily deduced from the data which have been stated in the preceding pages. 5 proportions of potash, equal to 240 grains, must be decomposed to form with an equal number of proportions

and THOMSON; and above all, in the decompositions by the VOLTAIC apparatus. Where oxygene and hydrogene, oxygene and inflammable bodies, acids and alkalies, &c. must separate in uniform ratios.

of oxymuriatic gas equal to 164.5 grains, 5 proportions of muriate of potash equal to 367 grains; and 5 of oxygene equal to 37, 5 grains, combined with one of potash, equal to 48, must unite in triple union with one of oxymuriatic gas equal to 32.9, to form one proportion, equal to 118.4 grains of hyperoxymuriate of potash.

### 3. *On the Combinations of the Metals of the Earths, with Oxygene and Oxymuriatic Gas.*

The muriates of baryta, lime, and strontia, after being a long time in a white heat, are not decomposable by any simple attractions: thus, they are not altered by dry boracic acid, though, when water is added to them, they readily afford muriatic acid and their peculiar earths.

From this circumstance, I was induced to believe that these three compounds consist merely of the peculiar metallic bases, which I have named barium, strontium, and calcium, and oxymuriatic gas; and such experiments as I have been able to make, confirm the conclusion.

When baryta, strontia, or lime, is heated in oxymuriatic gas to redness, a body precisely the same as a dry muriate is formed, and oxygene is expelled from the earth. I have never been able to effect so complete a decomposition of these earths by oxymuriatic gas, as to ascertain the quantity of oxygene produced from a given quantity of earth. But in three experiments made with great care I found that one of oxygene was evolved for every two in volume of oxymuriatic gas absorbed.

I have not yet tried the experiment of acting upon oxymuriatic gas by the bases of the alkaline earths; but I have

not the least doubt that these bodies would combine directly with that substance, and form dry muriates.

In the last experiments that I made on the metallization of the earths by amalgamation, I paid particular attention to the state of the products formed, by exposing the residuum of amalgams to the air. I found that baryta formed in this way was not fusible at an intense white heat, and that strontia and lime so formed gave off no water when ignited. Baryta made from chrystals of the earth, as M. BERTHOLLET has shewn, is a fusible hydrat, and I found that this earth gave moisture when decomposed by oxymuriatic gas; and the lime, in hydrat of lime, was much more rapidly decomposed by oxymuriatic gas than quicklime, its oxygene being rapidly expelled with the water.

Some dry quicklime was heated in a retort, filled with muriatic acid gas; water was instantly formed in great abundance, and it can hardly be doubted, that this arose from the hydrogene of the acid combining with the oxygene of the lime.

As potassium so readily decomposes common salt, I thought it might possibly decompose muriate of lime, and thus afford easy means of procuring calcium. The rapidity with which muriate of lime absorbs water, and the difficulty of freeing it even by a white heat from the last portions, rendered the circumstances of the experiments unfavourable. I found, however, that by heating potassium strongly, in contact with the salt, in a retort of difficultly fusible glass, I obtained a dark coloured matter, diffused through a vitreous mass, which effervesced strongly with water. The potassium had all disappeared, and the retort had received a heat at which potassium

entirely volatilizes. I had similar results with muriate of strontia, and (though less distinct, more potassium distilling off unaltered) with muriate of baryta. Either the bases of the earths were wholly or partially deprived of oxymuriatic gas in these processes, or the potassium had entered into triple combination with the muriates. I hope on a future occasion to be able to decide this point.

Combinations of muriatic acid gas with magnesia, alumine and silex, are all decomposed by heat, the acid being driven off, and the earth remaining free. I conjectured from this circumstance, that oxymuriatic gas would not expel oxygene from these earths, and the suspicion was confirmed by experiments. I heated magnesia, alumine, and silex to redness in oxymuriatic gas, but no change took place.

M. M. Gay Lussac and Thenard have shewn that baryta is capable of absorbing oxygene; and it seems likely, (as according to Mr. Chenevix's experiments, most of the earths are capable of becoming hyperoxymuriates) that peroxides of their bases must exist.

I endeavoured to combine lime with more oxygene, by heating it in hyperoxymuriate of potash, but without success, at least after this process it gave off no oxygene in combining with water. The salt, called oxymuriate of lime, made for the use of the bleachers, I found gave off oxygene by heat, and formed muriate of lime.

From the proportions which I have given in the last Bakerian lecture, but which were calculated from the analyses of sulphates, it follows that if the muriates of baryta, strontia, and lime, be regarded as containing one proportion

of oxymuriatic gas, and one of metal, then they would consist of 71* barium, 46 strontium, and 21 calcium, to 32.9 of oxymuriatic gas.

To determine how far these numbers are accurate, 50 grains of each of these muriates that had been heated to whiteness were decomposed by nitrate of silver, the precipitate was collected, washed, heated, and weighed.

The muriate of baryta, treated in this way, afforded 68 grains of horn-silver.

The muriate of strontia 85 grains.

The muriate of lime 125 grains.

From experiments to be detailed in the next section, it appears that horn-silver consists of 12 of silver to 3.9 of oxymuriatic gas, and consequently that barium should be represented by 65.1, strontium by 46.1, and calcium by 20.8.

### 4. *On the Combinations of the Common Metals, with Oxygene and Oxymuriatic Gas.*

In the limits which it is usual to adopt in this lecture, it will not be possible for me to give more than an outline of the numerous experiments that I have made on the combinations of oxymuriatic gas with metals; I must confine myself to a general statement of the mode of operating, and the results. I used in all cases small retorts of green glass, containing from 3 to 6 cubical inches, furnished with stopcocks. The metallic substances were introduced, the retort exhausted and filled with the gas to be acted upon, heat was applied by

* If Mr. JAMES THOMPSON's analysis of sulphate of barytes be made the basis of calculation, sulphuric acid being estimated as 36, then the number representing barium will be about 65.5.

means of a spirit lamp, and after cooling, the results were examined, and the residual gas analysed.

All the metals that I tried, except silver, lead, nickel, cobalt, and gold, when heated, burnt in the oxymuriatic gas, and the volatile metals with flame. Arsenic, antimony, tellurium and zinc with a white flame, mercury with a red flame. Tin became ignited to whiteness, and iron and copper to redness; tungsten and manganese to dull redness; platina was scarcely acted upon at the heat of fusion of the glass.

The product from arsenic was butter of arsenic; a dense, limpid, highly volatile fluid, a non-conductor of electricity, and of high specific gravity, and which when decomposed by water, gave oxide of arsenic and muriatic acid. That from antimony, was butter of antimony, an easily fusible and volatile solid, of the colour of horn-silver, of great density, crystallizing on cooling in hexahedral plates, and giving, by its decomposition by water, white oxide.

The product from tellurium, in its sensible qualities, resembled that from antimony, and gave when acted on by water white oxide.

The product from mercury was corrosive sublimate. That from zinc was similar in colour to that from antimony, but was much less volatile.

The combination of oxymuriatic gas and iron, was of a bright brown; but having a lustre approaching to the metallic, and was iridescent like the Elba iron ore. It volatilized at a moderate heat, filling the vessel with beautiful minute crystals of extraordinary splendour, and collecting in brilliant plates, the form of which I could not determine. When acted on by water, it gave red muriate of iron.

Copper formed a bright red brown substance, fusible at a heat below redness, and becoming crystalline and semi-transparent on cooling, and which gave a green fluid, and a green precipitate by the action of water.*

The substance from manganese was not volatile at a dull red heat; it was of a deep brown colour, and by the action of water became of a brighter brown: a muriate of manganese, which did not redden litmus remained in solution; and an insoluble matter remained of a chocolate colour.†

Tungsten afforded a deep orange sublimate, which, when decomposed by water, afforded muriatic acid, and the yellow oxide of tungster.

Tin afforded LIBAVIUS's liquor, which gave a muriate by the action of water containing the oxide of tin, at the maximum of oxidation.

Silver and lead produced horn-silver and horn-lead, and bismuth, butter of bismuth. The absorption of oxymuriatic gas was in the following proportions for two grains of each of the metals: for arsenic 3.6 cubical inches, for antimony

* It is worth enquiry, whether the precipitate from oxymuriate of copper by water is not a hydrated submuriate, analogous in its composition to the crystalized muriate of Peru. This last I find affords muriatic acid and water by heat.

The *resin of copper* discovered by BOYLE, formed by heating copper with corrosive sublimate, probably contains only 1 proportion of oxymuriatic gas, whilst that above referred to must contain 2.

† When muriate of manganese is made by solution of its oxide in muriatic acid, a neutral combination is obtained, but this is decomposed by heat; muriatic gas flies off, and brown oxide of manganese remains. In this respect manganese appears as a link between the ancient metals and the newly discovered ones. Its muriate is decomposed like that of magnesia; and its oxide is the only one amongst those long known, as far as my experiments have gone, which neutralizes the acid energy of muriatic acid gas, so as to prevent it in solution from affecting vegetable blues.

3.1, for tellurium 2.4, for mercury 1.05,* for zinc 3.2, for iron 5.8, for tin 4, for bismuth 1.5, for copper 3.4, for lead .9, for silver, the absorption of volume was $\frac{9}{10}$, and the increase of weight of the silver was equivalent to $\frac{6}{10}$ of a grain.†

In acting upon metallic oxides by oxymuriatic gas, I found that those of lead, silver, tin, copper, antimony, bismuth, and tellurium, were decomposed in a heat below redness, but the oxides of the volatile metals, more readily than those of the fixed ones. The oxides of cobalt and nickel were scarcely acted upon at a dull red heat. The red oxide of iron was not affected at a strong red heat, whilst the black oxide was rapidly decomposed at a much lower temperature; arsenical acid underwent no change at the greatest heat that could be given it in the glass retort, whilst the white oxide readily decomposed.

In cases where oxygene was given off, it was found exactly the same in quantity as that which had been absorbed by the metal. Thus 2 grains of red oxide of mercury absorbed $\frac{9}{10}$ of a cubical inch of oxymuriatic gas, nd afforded 0.45 of oxygene.‡ Two grains of dark olive oxide from calomel decom-

* The gas in these experiments was not freed from aqueous vapour, and as stopcocks of brass were used, a little gas might have been absorbed by the surface of this metal, so that the processes offer only approximations to the composition of the oxymuriates. The processes on lead, tellurium, iron, antimony, copper, tin, mercury, and arsenic, were carried on in three successive days, during which the height of the mercury in the barometer varied from 30.26 inches to 30.15, and the height of that in the thermometer from 63.5 to 61 FAHRENHEIT.

The experiment on silver was made at the temperature of 52 FAHRENHEIT, and under a pressure equal to that of 29.9 inches.

† This agrees nearly with another experiment made by my brother, Mr. JOHN DAVY, in which 12 grains of silver increased to 15.9 during their conversion into horn-silver.

‡ I have made two analyses of corrosive sublimate and calomel, with considerable

posed by potash, absorbed about $\frac{94}{100}$ of oxymuriatic gas, and afforded $\frac{24}{100}$ of oxygene, and corrosive sublimate was produced in both cases.

In the decomposition of the white oxide of zinc, oxygene was expelled exactly equal to half the volume of the oxymuriatic acid absorbed. In the case of the decomposition of the black oxide of iron, and the white oxide of arsenic, the changes that occurred were of a very beautiful kind; no oxygene was given off in either case, but butter of arsenic, and arsenical acid formed in one instance, and the ferruginous sublimate, and red oxide of iron in the other.

care. I decomposed 100 grains of corrosive sublimate, by 90 grains of hydrat of potash. This afforded 79.5 grains of orange coloured oxide of mercury, 40 grains of which afforded 9.15 cubical inches of oxygene gas; the muriate of silver formed from the 100 grains was 102.5.

100 grains of calomel, decomposed by 90 grains of potash, afforded 82 grains of olive coloured oxide of mercury, of which 40 grains gave by decomposition by heat 4.8 cubical inches of oxygene. The quantity of horn-silver formed from the 100 grains was 58.75 grains.

In the second analysis, the quantity of oxide obtained from corrosive sublimate was 78.7; the quantity of muriate of silver formed was 103.4; the oxide produced from calomel weighed 83 grains; the horn-silver formed was $57\frac{1}{2}$ grains. I am inclined to put most confidence in the last analyses; but the tenor of both is to shew that the quantity of oxymuriatic gas in corrosive sublimate, is exactly double that in calomel, and that the orange oxide contains twice as much oxygene as the black, the mercury being considered as the same in all. The olive colour of the oxide formed from calomel, is owing to a slight admixture of orange oxide, formed by the oxygene of the water used in precipitation; the tint I find is almost black, when a boiling solution of potash is used; and trituration, with a little orange oxide brings the tint to olive. It has been stated, that the olive oxide thrown down from calomel by potash is a submuriate; but I have never been able to find a vestige of muriatic acid in it when well washed. It is not easy to obtain perfect precision in analyses of the oxides of mercury; water adheres to the oxides, which cannot be entirely driven off without the expulsion of some oxygene. In all my experiments, though the oxides had been heated to a temperature above 212, a little dew collected in the neck of the retort, so that the 40 grains must have been over-rated.

Two grains of white oxide of arsenic absorbed 0.8 of oxymuriatic gas.*

I doubt not that the same phenomena will be found to occur in other instances, in which the metal has comparatively a slight attraction only for oxymuriatic gas, and when it is susceptible of different degrees of oxydation, and in which the peroxide is used.

The only instance in which I tried to decompose a common metallic oxide, by muriatic acid, was in that of the fawn coloured oxide of tin; water rapidly separated, and LIBAVIUS's liquor was formed.

From the proportions which may be gained in considering the volumes of oxymuriatic gas absorbed by the different metals, in their relations to the quantity of oxygene which would be required to convert them into oxides, it would appear, that in the experiments to which I have referred, either one, two, or three proportions of oxymuriatic gas combine with one of metal, and consequently, from the composition of the muriates, it will be easy to obtain the numbers representing the proportions in which these metals may be conceived to enter into other compounds.†

* A singular instance of the tendency of the oxide of arsenic to become arsenical acid, occurs in its action on fused hydrat of potash, the water in the hydrat is rapidly decomposed, and arseniuretted hydrogene evolved, and arseniate of potash formed.

† From the experiments detailed in the note in the opposite page, it would appear that the number representing the proportion in which mercury combines must be about 200. That of silver, as would appear from the results, page 25, about 100. The numbers of other metals may be learnt from the data in the same page, but from what has been stated, these data cannot be considered as very correct.

### 5. *General Conclusions and Observations, illustrated by Experiments.*

All the conclusions which I ventured to draw in my last communication to the Society, will, I trust, be found to be confirmed by the whole series of these new enquiries.

Oxymuriatic gas combines with inflammable bodies, to form simple binary compounds; and in these cases, when it acts upon oxides, it either produces the expulsion of their oxygene, or causes it to enter into new combinations.

If it be said that the oxygene arises from the decomposition of the oxymuriatic gas, and not from the oxides, it may be asked, why it is always the quantity contained in the oxide; and why in some cases, as those of the peroxides of potassium and sodium, it bears no relation to the quantity of gas?

If there existed any acid matter in oxymuriatic gas, combined with oxygene, it ought to be exhibited in the fluid compound of one proportion of phosphorus, and two of oxymuriatic gas; for this, on such an assumption, should consist of muriatic acid (on the old hypothesis, free from water) and phosphorous acid; but this substance has no effect on litmus paper, and does not act under common circumstances, on fixed alkaline bases, such as dry lime or magnesia. Oxymuriatic gas, like oxygene, must be combined in large quantity with peculiar inflammable matter, to form acid matter. In its union with hydrogene, it instantly reddens the driest litmus paper, though a gaseous body. Contrary to acids, it expels oxygene from protoxides, and combines with peroxides.

When potassium is burnt in oxymuriatic gas, a dry compound is obtained. If potassium combined with oxygene is

employed, the whole of the oxgene is expelled, and the same compound formed. It is contrary to sound logic to say, that this exact quantity of oxygene is given off from a body not known to be compound, when we are certain of its existence in another; and all the cases are parallel.

An argument in favour of the existence of oxygene in oxymuriatic gas, may be derived by some persons from the circumstances of its formation, by the action of muriatic acid on peroxides, or on hyperoxymuriate of potash; but a minute investigation of the subject will, I doubt not, shew that the phænomena of this action are entirely consistent with the views I have brought forward. By heating muriatic acid gas in contact with dry peroxide of manganese, water I found was rapidly formed, and oxymuriatic gas produced, and the peroxide rendered brown. Now as muriatic acid gas is known to consist of oxymuriatic gas and hydrogene, there is no simple explanation of the result, except by saying that the hydrogene of the muriatic acid, combined with oxygene from the peroxide to produce water.

Scheele explained the bleaching powers of the oxymuriatic gas, by supposing that it destroyed colours by combining with phlogiston. Berthollet considered it as acting by supplying oxygene. I have made an experiment, which seems to prove that the pure gas is incapable of altering vegetable colours, and that its operation in bleaching depends entirely upon its property of decomposing water, and liberating its oxygene.

I filled a glass globe containing dry powdered muriate of lime, with oxymuriatic gas. I introduced some dry paper tinged with litmus that had been just heated, into another globe containing dry muriate of lime; after some time this globe

was exhausted, and then connected with the globe containing the oxymuriatic gas, and by an appropriate set of stopcocks, the paper was exposed to the action of the gas. No change of colour took place, and after two days there was scarcely a perceptible alteration.

Some similar paper dried, introduced into gas that had not been exposed to muriate of lime, was instantly rendered white.*

Paper that had not been previously dried, brought in contact with dried gas, underwent the same change, but more slowly.

The hyperoxymuriates seem to owe their bleaching powers entirely to their loosely combined oxygene; there is a strong tendency in the metal of those in common use, to form simple combinations with oxymuriatic gas, and the oxygene is easily expelled or attracted from them.

It is generally stated in chemical books, that oxymuriatic gas is capable of being condensed and crystallized at a low temperature; I have found by several experiments that this is not the case. The solution of oxymuriatic gas in water freezes more readily than pure water, but the pure gas dried by muriate of lime undergoes no change whatever, at a temperature of 40 below 0° of FAHRENHEIT. The mistake seems to have arisen from the exposure of the gas to cold in botttles containing moisture.

I attempted to decompose boracic and phosphoric acids by oxymuriatic gas, but without success; from which it seems probable, that the attractions of boracium and phosphorus for

* The last experiments were made in the laboratory of the Dublin Society; most of the preceding ones in the laboratory of the Royal Institution; and I have been permitted to refer to them by the Managers of that useful public establishment.

oxygene are stronger than for oxymuriatic gas. And from the experiments I have already detailed, iron and arsenic are analogous in this respect, and probably some other metals.

Potassium, sodium, calcium, strontium, barium, zinc, mercury, tin, lead, and probably silver, antimony, and gold seem to have a stronger attraction for oxymuriatic gas than for oxygene.

I have as yet been able to make very few experiments on the combinations of the oxymuriatic compounds with each other, or with oxides. The liquor from arsenic, and that from tin, mix, producing an increase of temperature; and the phosphuretted, and the sulphuretted liquors unite with each other, and with the liquor of LIBAVIUS, but without any remarkable phenomena.

I heated lime gently in a green glass tube, and passed the phosphoric sublimate, the saturated oxymuriate of phosphorus through it, in vapour; there was a violent action with the production of heat and light, and a gray fused mass was formed, which afforded by the action of water, muriate and phosphate of lime.

I introduced some vapour from the heated phosphoric sublimate, into an exhausted retort containing dry paper tinged with litmus: the colour slowly changed to pale red. This fact seems in favour of the idea that the substance is an acid; but as some minute quantity of aqueous vapour might have been present in the receiver, the experiment cannot be regarded as decisive: the strength of its attraction for ammonia, is perhaps likewise in favour of this opinion. All the oxymuriates that I have tried, indeed form triple compounds with this alkali; but phosphorus is expelled by a gentle

heat from the other compounds of oxymuriatic gas and phosphorus with ammonia, and the substance remaining in combination is the phosphoric sublimate.

### 6. *Some Reflections on the Nomenclature of the Oxymuriatic Compounds.*

To call a body which is not known to contain oxygene, and which cannot contain muriatic acid, oxymuriatic acid, is contrary to the principles of that nomenclature in which it is adopted; and an alteration of it seems necessary to assist the progress of discussion, and to diffuse just ideas on the subject. If the great discoverer of this substance had signified it by any simple name, it would have been proper to have recurred to it; but, dephlogisticated marine acid is a term which can hardly be adopted in the present advanced æra of the science.

After consulting some of the most eminent chemical philosophers in this country, it has been judged most proper to suggest a name founded upon one of its obvious and characteristic properties——its colour, and to call it *Chlorine*, or *Chloric* gas.*

Should it hereafter be discovered to be compound, and even to contain oxygene, this name can imply no error, and cannot necessarily require a change.

Most of the salts which have been called muriates, are not known to contain any muriatic acid, or any oxygene. Thus LIBAVIUS's liquor, though converted into a muriate by water, contains only tin and oxymuriatic gas, and horn-silver seems incapable of being converted into a true muriate.

I venture to propose for the compounds of oxymuriatic

* From χλωρος.

gas and inflammable matter, the name of their bases, with the termination *ane*. Thus argentane may signify horn-silver; stannane, LIBAVIUS's liquor; antimonane, butter of antimony; sulphurane, Dr. THOMSON's sulphuretted liquor; and so on for the rest.

In cases when the proportion is one quantity of oxymuriatic gas, and one of inflammable matter, this nomenclature will be competent to express the class to which the body belongs, and its constitution. In cases when two or more proportions of inflammable matter, combine with one of gas; or two or more of gas, with one of inflammable matter, it may be convenient to signify the proportions by affixing vowels before the name, when the inflammable matter predominates, and after the name, when the gas is in excess; and in the order of the alphabet, *a* signifying two, *e* three, *i* four, and so on.

The name muriatic acid, as applied to the compound of hydrogene and oxymuriatic gas, there seems to be no reason for altering. And the compounds of this body with oxides should be characterized in the usual manner, and as the other neutral salts.

Thus muriate of ammonia and muriate of magnesia, are perfectly correct expressions.

I shall not dwell any longer at present upon this subject.—What I have advanced, I advance merely as suggestion, and principally, for the purpose of calling the attention of philosophers to it.* As chemistry improves, many other alterations

* It may be conceived that a name may be found for the oxymuriatic gas in some modification of its present appellation which may harmonize with the new views, and which may yet signify its relation to the muriatic acid, such as demuriatic gas, or oxymuric gas; but in this case it would be necessary to call the muriatic acid, hydrogenated muriatic acid, or hydromuriatic acid; and the salts which contain it

will be necessary; and it is to be hoped that whenever they take

hydrogenated muriates or hydromuriates; and on such a plan, the compounds of oxymuriatic gas must be called demuriates or oxymuriates, which I conceive would create more complexity and difficulty in unfolding just ideas on this department of chemical knowledge than the methods which I have ventured to propose. It may however be right, considering the infant state of the investigation, to suspend, for a a time, the adoption of any new terms for these compounds. It is possible that oxymuriatic gas may be compound, and that this body and oxygene may contain some common principle; but at present we have no more right to say that oxymuriatic gas contains oxygene than to say that tin contains hydrogene; and names should express things and not opinions; and till a body is decompounded, it should be considered as simple.

In the last number of Mr. Nicholson's Journal, which appeared February 1st, whilst this sheet was correcting for the press, I have seen an ingenious paper, by Mr. Murray, of Edinburgh, in which he has attempted to shew, that oxymuriatic gas contains oxygene. His methods are, by detonating oxymuriatic gas in excess, with a mixture of hydrogene, and gaseous oxide of carbone, when he *supposes* carbonic acid is formed; and by mixing oxymuriatic gas in excess, with sulphuretted hydrogene, when he *supposes* sulphuric acid, or sulphureous acid is formed. In some experiments, in which my brother, Mr. John Davy, was so good as to co-operate, made over boiled mercury, we found, that 7 parts of hydrogene, 8 parts of gaseous oxide of carbone, and 20 parts of oxymuriatic gas, exploded by the electric spark, diminished to about 30 measures; and calomel was formed on the sides of the tube. On adding dry ammonia in excess, and exposing the remainder to water, a gas remained, which equalled more than 9 measures, and which was gaseous oxide of carbone, with no more impurity than might be expected from the air in the gasses, and the nitrogene expelled from the ammonia; so that the oxygene in Mr. Murray's carbonic acid, it seems, was obtained from *water*, or from the carbonic oxide. Sulphuretted hydrogene, added over dry mercury, to oxymuriatic gas in excess, inflamed in two or three experiments; muriatic acid gas containing the vapour of oxymuriate of sulphur, was formed, which, when neutralized by ammonia, gave muriate of ammonia, and a combination of ammonia, and oxymuriate of sulphur.

When a mixture of oxymuriatic gas in excess, and sulphuretted hydrogene, was suffered to pass into the atmosphere, the smell was that of oxymuriate of sulphur; there was not the slightest indication of the presence of any sulphuric or sulphureous acid. If Mr. Murray had used ammonia, instead of water, for analyzing his results, I do not think he would have concluded, that oxymuriatic gas is capable of decomposition by such methods.

I shall not, at present, enter upon a detail of other experiments which I have made

place, they will be made independent of all speculative views, and that new names will be derived from some simple and invariable property, and that mere arbitrary designations will be employed, to signify the class to which compounds or simple bodies belong.

on this subject, in co-operation with my brother, as it is his intention to refer to them, in an answer to Mr. MURRAY's paper.

I shall conclude, by saying, that this ingenious chemist, has mistaken my views, in supposing them hypothetical; I merely state what I have seen, and what I have found. There *may* be oxygene in oxymuriatic gas; but I can find none. I repeated Mr. MURRAY's experiments with great interest; and their results, when *water* is excluded, entirely confirm all my ideas on the subject, and afford no support to the hypothetical ideas, which he has laboured so zealously to defend.